conception of a cylinder of syenite enfolding anorthosite is therefore neither a necessary nor a true one; rather, there are a number of separate syenite masses.

ADIRONDACK INTRUSIVES

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In his paper on the "Structure of the Anorthosite Body in the Adirondacks" Professor Cushing offers some objections to the interpretation of Adirondack igneous geology that was given by me in the paper "The Problem of the Anorthosites," and he has kindly asked me to comment upon his objections. It naturally gives me considerable satisfaction that an investigator with Professor Cushing's broad experience of Adirondack geology should accept the more important and vital aspects of my interpretation of the genesis of Adirondack igneous types. I therefore find myself disinclined to object very vigorously to his remarks on features of Adirondack structure concerning which he finds it necessary to disagree with me. This is especially true since it would be presumptuous on my part to differ from him on any point involving actual knowledge of field facts. Nevertheless, there seem to be certain questions of interpretation on which there is room for alternative views.

The common, basic border phase of the anorthosite Professor Cushing considers fatal to the idea of the extension of that rock type laterally as a sheetlike mass beyond the limits of its present exposure. He accepts Daly's interpretation of this border phase as a chilled portion and considers that this phase must be the outer limit of the anorthosite. I, too, accept Daly's interpretation of the basic border, but consider that it is not necessarily an outer limit; it may be an upper limit, or rather a former upper limit. It may therefore represent a chilled upper portion of a laccolithic mass extending far beyond the limits of its present exposure.

It is perhaps necessary to go into this matter in greater detail, and, in order that this may be done, mention will first be made of

a much simpler example of the same phenomenon. In the Palisade diabase of New Jersey gravitative differentiation has taken place with the result that there has been formed in the lower layers an olivine-rich diabase and in the upper layers more acidic types, in local patches verging upon granite. At the upper border, however, a more basic phase occurs which contains a small amount of olivine and represents the original magma quickly chilled and undifferentiated. In this body of moderate dimensions all the differentiates have remained in position, except that the acidic phase may be injected occasionally into the more basic varieties as aplitic dikes. When this occurs, the acidic phase has been noted to exert a particularly strong corrosive or recrystallizing action on the basic phase.

While the differentiates are of other types in the case of the Adirondack complex, I believe that in a broad way the relations are substantially the same, the principal complicating circumstance being the prominence of reintrusion of the later liquid, the syenite. The gabbro border phase I believe, with Daly and Cushing, to be a chilled border, and, while this matter was not discussed in connection with the Adirondacks, mention was made of such chilled phases on page 213. In an undisturbed mass the syenite would everywhere lie immediately below this basic phase if the mass had also a very regular contact. However—and this brings us to another of Professor Cushing's objections—if the mass had an irregular upper contact, the syenite need be present only in the re-entrants of the roof and need not therefore form a continuous border about the anorthosite. Add to this the fact that the syenite has been disturbed and re-intrusion has occurred, and I think that this fact will become still more obvious. It must be confessed that Professor Cushing was perhaps justified in considering a continuous syenite body a necessary consequence of my hypothesis on account of the diagrams that were offered in illustration of the conception. But these were intended to represent in a diagrammatic way the conditions under which the various types were generated, and not to give a picture, except in a generalized way, of the actual distribution of types in the Adirondacks at present. It is recognized that reintrusion of the syenite occurred, resulting in satellitic bodies

at higher horizons in the Grenville, though much of it remained substantially where generated—enough, perhaps, to justify the statement that the Adirondack complex is "*essentially* a sheetlike mass with syenite overlying anorthosite." Whether distant syenite masses are to be regarded as related to the anorthosite I cannot say.

As a consequence of reintrusion, invasion of the anorthosite by syenite, in so far as this occurs, is especially likely to be true of the basic border phase, the anorthosite-gabbro or gabbro, and, after the manner of the acidic phases in the Palisade intrusive, it may be expected that the syenite will exert a strong corrosive or resorbing action on these basic differentiates, such as that described by Professor Cushing from Long Lake. While, therefore, the syenite would be pushed up from below into and beyond the basic phase of the anorthosite, it is considered that the syenite came into being at a higher level than the anorthosite proper. This is not inconsistent with the occasional occurrence of dikes of syenite in the anorthosite, for a splitting of the solidified anorthosite would permit the formation of such dikes from an overlying liquid syenite as readily as from a deeper-seated mass. Professor Cushing is able to bring into court more examples of these dikes than I had supposed were known, but I think that it must be admitted that in much of the quadrangle work the syenite is considered later than the anorthosite solely on the basis of his findings in the Long Lake quadrangle. This might be considered as due to failure of exposure, but in the same areas there is no lack of evidence of the invasion of the Grenville by syenite. I consider it likely, therefore, that the syenite does not invade the anorthosite in exactly the same way, but is largely transitional into it, although, being of somewhat later consolidation, it may send dikes into the anorthosite on occasion. My observations are admittedly limited, but I do not think that the intermediate types to be seen at Lake Placid are formed by interaction of the two types, an action of which Professor Cushing finds abundant evidence at Long Lake. The Placid types are quite definitely intermediate between syenite and anorthosite, not between syenite and gabbro, as are Cushing's reaction types.

In conclusion, I would state that, while Professor Cushing has raised legitimate objections and there is certainly room for difference

of opinion, it still seems to me to be advisable to keep an open mind on the possibility that the syenite and anorthosite occur "*substantially* as layers with the syenite above." It is especially desirable in view of the fact, recognized by Professor Cushing, that the anorthosite occurs in the more deeply eroded portions and the syenite principally at higher horizons, an arrangement not easily reconciled with the opinion that the syenite pushes up into the anorthosite from below.

ADIRONDACK INTRUSIVES

H. P. CUSHING

I am greatly indebted to Dr. Bowen for his additional contribution to this discussion. I take the liberty of considering briefly the points he brings out.

He suggests that the chilled gabbro border of the anorthosite is not a lateral border but a remnant of an upper one. It is very difficult for me clearly to visualize the structure of the region on this view. It is, roughly, about 100 miles across the mid-Adirondack region from east to west, and, again roughly, the easterly half of this distance is occupied by pretty clean anorthosite, and the westerly half contains a great number of syenite bodies and no anorthosite at all. The chilled gabbro border is about midway of the region. If it is a chilled upper portion of a laccolith, consisting of pyroxenite and gabbro below, then anorthosite, then syenite, and, finally, the chilled gabbro roof, since tilted so that the present erosion surface cuts it at a considerable angle, it is necessary to conceive that this chilled upper surface passes below ground in the westerly direction and into the air to the east. Under such a view the present-day syenite masses of the west must have broken through this cover to reach their present position, and there is no particular difficulty in imagining that they did so. But under this view it seems to me necessary that we should also find syenite to the east of the chilled border and close to it—that syenite which formed as a differentiate in the upper part of the chamber, underneath the chilled upper surface. Even if the upper part was very